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specialization, in pronouncing upon a critical question in the classification of the fossiliferous rocks.

JOHN M. CLARKE.

THE PROBLEM OF COLOR.

ALTHOUGH I don't accept Professor Cattell's contention, in the last number of the *Psychological Review*, that the nugatory process by which two colored lights (if properly chosen in hue and in intensity) disappear for sensation and leave behind a sense of grayness only is due to a cortical and not to a retinal physiological process, I am nevertheless willing (in the interest of fair play) to furnish him with one more reason on his side. When a colored object is mirrored in a piece of colored glass (say red in blue), we get in general a color blend, that is, for consciousness, a reddish-blue sensation. In case the colors chosen are a pair which, on fusing, are transformed into something else (yellow and blue into white, or red and green into yellow), this is, according to all the non-psychical color-theories, because two counteracting color-processes in the retina are exactly balanced, or else because two partial photo-chemical molecular dissociations unite to complete each other and to produce an undifferentiated gray-process,—either of these suppositions being sufficiently plausible in itself. But—and this is the fact, if it is a fact, which works upon Professor Cattell's side—there are occasions upon which, according to Helmholtz and to Wundt, this antagonism, or this completion, fails to take place. One sometimes sees, they say, one color *through* the other; guided by the belief that the red sensation is due to the presence of a red book, *e. g.*, one cannot help but *see* the redness of the book through the sea of blue. They do not dwell upon the colors which they used in making the experiment—so long as these are red and blue there is nothing strange in the differing interpretations; but if, under these circumstances, blue and yellow should not give white (and red and green should not give yellow), then it would seem to follow that the antagonistic or the completing processes are not of the nature of chemical changes in the retina—such could not be so easily undone by the reasoning, or the

perceiving, Psyche. Hering denies with great warmth the contention of Helmholtz and of Wundt that these exceptional cases occur; or rather, he says that if they do occur it is owing to spots or unevennesses in one or the other of the two surfaces. But even though she be assisted by any ulterior aids whatever, it would not seem that the Psyche can undo, in the interests of reasonable interpretation, a chemical change that has already taken place. Perhaps she can, however; but in that case her powers must also suffice to undo an *actual* white (or yellow) and separate it into its possible components. If, in the case of a blue book seen in a yellow glass, for a portion in the center of the surface of the book a gray of equal brightness be substituted, and a like gray for an exactly coinciding portion of the yellow reflector, then it is possible that self-deception would go so far as to enable us to see a continuous blue book in a continuous yellow mirror. The experiment is perhaps worth trying.

On the other hand (to be equally fair to my own side, in turn), the fact that *binocular* color mixture does not occur to any great extent—that is, does not occur for colors far apart in the spectrum—is at once destructive to any hypothesis which relegates the fusion of colors to the perception-forming centers of the brain. Whether an overlapping blue and yellow are mediated by one eye or by two can have nothing to do with the case if their mutual quenching is an affair of perception. Helmholtz, after a long series of the most painstaking experiments, declared absolutely that binocular color-fusion does not take place.* This shows, in passing, the unprejudiced character of his work, for the fact, as I have said, is quite destructive to his theory that the mutual suppression of blue and yellow into white is merely a matter of the judgment: it cannot make any difference whether we know that we see blue and yellow at once through one nasal half-retina, or through a nasal and a temporal half-retina together—the more so as we have in general

* Binocular color-fusing of two complementary colors may be obtained with the Hering color-mixer by 'long and steady gazing,' but this is the sufficient condition for turning each color into a dead gray, when looked at by itself.

absolutely no consciousness as to which eye we are seeing anything with.

It is customary to speak of color-mixing as if it were the same sort of thing throughout the whole spectrum, but in reality it is of two very different kinds. When a unitary green and blue are mixed to produce a blue-green, the phenomenon is purely a psychological one (and there is nothing strange in the fact that such mixtures work binocularly as well as monocularly); we can see in the blue-green the blue and the green of which it is composed (and we have not even in this case taken the trouble to devise a separate name for it). But if a spectral red and a spectral green in neither of which any trace of yellow can be detected be seen together (and even if one of them is a trifle bluish), a yellow is produced which has not any perceptible falling off, even in saturation, from the yellow of the spectrum (as has just been stated explicitly by Breuer and von Kries); and a correspondingly strange event results from the mixing of blue and yellow. To say that such a transformation-scene as this is the work of judgment (the judgment being led to it by no motive whatever—it cannot be anything in reality, it would seem, but the pure spontaneous play of fancy, rather than the work of a reasons-obeying judgment, or perception)—this is to make a serious draft upon the powers with which we need to endue that faculty, or, to use the more modern term, that cortical center. At all events, the two occurrences are very different, and my object now is merely to suggest that they should be called by different names. When green and yellow producing ether-radiations are thrown together upon the retina, I would propose that the yellow-green sensation which results, be called a *color-blend*, and that the two colors be said to be blended. But when yellow and blue unite to make gray, I should say, using in fact a term of Helmholtz's, that the process is one of mutual *color-quenching* (and in the same way red and green may be said to quench each other when they result in yellow). Color-blending is plainly a psychological matter; color-quenching it is far more natural, in the first instance, to attribute to a peculiarity of the photo-chemical processes which we know to be going on in the retina.

Farther—still in the interest of mutual comprehensibility between the adherents of different schools, who speak at present languages which have too little in common—I would propose to call red, yellow, blue, and green, not primary, nor elementary, nor fundamental colors—that commits one to one or other of the rival schools; not 'principal' colors—that is purely an æsthetic designation; but *unitary* colors. Since the admirable discussion of this subject by Professor Elias Müller (*Ztsch. f. Psychol.*, Vols. X. and XIV.) no one can doubt—even of those who doubted it before—that these particular ether-radiations have for consciousness a peculiar character—that of being the end-members of 'rectilinear' color-series (series such that each member differs from the one before it *in the same way* in which that differs from the one next preceding); in other words, they are not, for consciousness, of the nature of *color-blends*. Yellow-green and green-blue are—on their faces—color-blends. Orange and violet have secured unitary names for themselves (though they are nothing but a reddish yellow and a reddish blue)—doubtless on account of the excessive interest which attaches to reds in nature as compared with greens; but that is not sufficient to make them unitary colors. This nomenclature commits one to no theory whatever—whether retinal or cortical; it is simply the expression of the psychological fact that there are four very characteristic points in the color gamut, red, yellow, green and blue, their character being sufficiently described by the word *unitary*. That this is true will easily be seen by any one who will take the trouble to spread out for himself in order in a circle as many different color-hues (all of the same saturation and the same brightness—the spectrum will not do, therefore), as can be procured.

To conclude, a color-blend is then surely a psychological product; an instance of color-quenching is either psychical or physiological according to the theory which one is pleased to adopt. How hard it is for the physicists to understand this point of view is evidenced by the fact that they are constantly affirming that fresh proof has been adduced of the Young-Helmholtz theory, because it has been shown that all the colors of the rainbow and white

besides can be made out of the *physical* mixture of red and green and blue. That fact has been put beyond doubt, once for all, by the exceedingly exact measurements of Professor König, made by means of an instrument of very ingenious construction (and so expensive that it has been duplicated for hardly any other laboratories). There is not a psychologist who denies this physical fact, and for the physicist to constantly re-affirm it, and to say that it has received fresh proof (see the report of the last meeting of the scientific societies in New York) is much the same as if he should valiantly affirm that one side of a shield is of silver by way of opposition to those who say that the other side is of gold. What the psychologist denies is not that gray results when blue and yellow are mixed upon the color wheel—he has admitted that long ago, and it will be found as an elementary statement in every text-book of psychology. But he refuses to admit, nevertheless, that white is an even red-green-blue *sensation* in the same sense in which purple is an even red-blue sensation. It is here that the adherents of the Young-Helmholtz theory should attack him.

C. LADD FRANKLIN.

A LARGE CRYSTAL OF SPODUMENE.

TO THE EDITOR OF SCIENCE: There has recently appeared in some scientific journals a notice of a crystal of spodumene stated to be about twenty-nine feet long, and to be the largest known. It may be of interest to your readers to learn that a much larger crystal has been observed. In the year 1885 while studying the tin ore or cassiterite localities of the Black Hills of Dakota I saw and measured, in the Etta tin mine near Harney's Peak, a spodumene crystal thirty-eight feet and six inches in length and thirty-two inches in thickness. This thirty-eight and a half foot crystal was almost perfect, and was situated within a few yards of the surface. Owing to its size and the difficulties of transportation at that time, the railway being one hundred and thirty miles distant, I made no attempt to have the crystal removed. I, however, collected other crystals of spodumene in the vicinity, and some of these measured from

two to six feet in length. Subsequently, in a public lecture upon the Black Hills, given in the University of North Dakota in February, 1886, I announced the discovery of the afore-said gigantic crystal; but, because of the pressure of teaching and other numerous duties, that discovery has not been reported in the regular scientific journals.

For the benefit of some readers it may perhaps be well to state that spodumene is a grayish-white or pink mineral of considerable hardness, being nearly as hard as quartz, and that it consists of silica, alumina and lithium.

HENRY MONTGOMERY.

TRINITY UNIVERSITY, TORONTO,
July 17, 1900.

UNITS AT THE INTERNATIONAL ELECTRICAL CONGRESS.*

AT the suggestion of Professor Hospitalier, Section I. of the Congress agreed that the following should be the members of the Commission on Units: Messrs. Ayrton (Great Britain), De Chatelain (Russia), Dorn (Germany), De Fodor (Hungary), Eric Gérard (Belgium), Hospitalier (France), Lombardi (Italy), Kennelly (United States); and at the first meeting of the Commission, on August 21st, which was attended also by Professor F. Kohlrausch and Sir W. Preece—whose names had been added to the list of the government delegates for Germany and England—a report presented to the Congress by the American Institute of Electrical Engineers was taken into consideration. This report had been drawn up for that Institute by a committee appointed for this purpose, and it contained the following resolutions:

(1) We consider that it is necessary to give names to the absolute units in the electromagnetic and electrostatic systems, as well as convenient prefixes to designate the decimal multiples and submultiples of these units in addition to those already in use.

(2) The International Congress of Electricians, which will take place this year in Paris, should be invited to choose the names and the prefixes.

(3) A great advantage would be gained by a rationalization of the electric and magnetic

*From *Nature*.